

RESPONSES TO STADDON, SHIMP, MALONE, AND DONAHOE

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I stand by my review. I documented all of my observations with quotations from the book. My speculations about motives were only in keeping with my wonder at the meanness of the treatment of Skinner. If the review contains some ad hominem elements, the ad hominem character of Staddon's attacks on Skinner invited them. If his goal was a presentation of behaviorism to nonbehaviorists, as he claims, he went about it in a strange way, devoting most of the book, not to "setting a context," but to trashing Skinner's writings and then presenting briefly his own idiosyncratic view, which I again say is indistinguishable from the tradition begun by Hull and Tolman and embraced by cognitive psychology today. What happened to constructive approaches like: *Skinner made conceptual contributions that took the science of behavior so far; many contributions have been made since, and here is the spectrum of views that one may find in contemporary behaviorism?* The book is far from a balanced presentation, but is a piece of advocacy, pushing a regressive version of behavior analysis. Worst, even here in this response, Staddon tells us that behaviorism has in no way advanced since Skinner. With this, he denies the contributions of thinkers like Howard Rachlin (1994), Philip Heline (1984), Mecca Chiesa (1994), Vicki Lee (1988), and myself (Baum, 2003/1994), who have disagreed with Skinner and worked to take behaviorism beyond Skinner's narrow and ambiguous conception. Although Staddon claims to "set a context" by criticizing Skinner's popular writings, much of the book is devoted to remonstrating about Skinner's views on theory, which, seen in historical perspective, were appropriate for their time. Staddon refuses to acknowledge this because he paints himself as an "iconoclast." He would save behavior analysis from irrelevance by reinstating a proper emphasis on theory.

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What sort of theory? Not the sort that Skinner advocated and that now is blossoming, but the very sort that has proven futile. He tells us that behavior is not what an organism does, but what a theoretical model does. He criticizes the competence-performance distinction but then instantiates it himself. I say again, the book is about theory. It promotes Staddon's view of the place of theory. It denigrates the research of other behavior analysts. It is not about any "new" behaviorism.

Shimp

Shimp's commentary is not about my review or Staddon's response. My review, like the book, is mostly about Staddon's treatment of Skinner. Staddon and I differ over what sorts of theory and explanation are helpful for behavior analysis, and that might be what triggered Shimp's discussion of issues about which he has differed with me before. Although it is tangential, I will respond nevertheless.

Failure on my part to explain molecular and molar paradigms clearly in my 2002 paper may have caused Shimp's misunderstanding. Apparently I underestimated the possibilities for confusion between the molecular-molar distinction and the local-extended (Shimp calls it "local-global") distinction. Two recent papers may help to clarify (Baum, 2004; Baum & Davison, 2004). In brief, the difference between the molecular and molar paradigms is a difference between conceptions of what behavior is, how to measure it, and how to construct theories about it. The first paper (Baum, 2004) explains that the molecular paradigm descends from associationism. It is the view that behavior (consciousness) is composed of discrete responses (ideas) that link together to produce complex units and sequences. Theories conceived in the molecular paradigm emphasize contiguity and immediate causes. The molar paradigm is newer, but goes back at least to 1896 when John Dewey published a famous article criticizing

the reflex and advocating instead a view of behavior as composed of “ coordinations ” that are continuous.

In my 2002 article, I advanced a view of behavior as composed of continuous activities that are wholes always composed of parts that are themselves less extended activities. Thus the molar paradigm sees behavior as nested continuous activities, which means that they have the property of *scale* (Buege, 1997). An activity that is a part of a more extended activity has smaller scale than the more extended activity. The smaller the scale, the more local is the analysis. The molar paradigm thus supports analysis at any level of extendedness, depending on one’s purposes (cf. Hineline, 2001). The other recent paper (Baum & Davison, 2004) illustrates this flexibility by analyzing data from an experiment on the dynamics of reinforcement at several different levels of extendedness. Shimp shows no awareness of the studies of local dynamics that Davison and I have published (Davison & Baum, 2000, 2002, 2003).

Finally, episodes of an activity are discrete events but fundamentally different from discrete responses. In the molecular view, duration is conceived only to be an attribute of discrete responses, whereas in the molar view, time is the primary measure and cumulates. In the molecular view, reinforcers are conceived always to follow discrete responses, immediately or at a delay, whereas in the molar view, reinforcers may follow an activity but usually accompany the activity, several or many occurring within an episode (e.g., watching television or having sex). Contra Shimp, my reading of Staddon’s research suggests to me that Staddon (e.g., 1983) typically works within a molar paradigm and that he and I might agree about some of these issues.

Shimp disagrees with my use of the word paradigm, pointing out that it has fostered a pernicious relativism. In rejecting relativism, however, one may still recognize that Kuhn’s (1970) concept of a paradigm is useful, particularly if it is distinguished from a theory conceived within the paradigm—that is, a theory based on the fundamental ontological and epistemological assumptions of the paradigm. At the least, clarifying a paradigm brings those fundamental assumptions into view.

Shimp suggests that my review was some-

how “ unfair. ” He never clarifies exactly what he means. Was it unfair for me to complain about Staddon’s distorted representation of Skinner’s contributions? Or Staddon’s shabby treatment of Skinner’s writings about theory? Was it unfair for me to point out that Staddon’s view of theory is indistinguishable from that of cognitive psychology? Or that his instantiation of theory had been published multiple times already and doesn’t even concern individuals or magnitude of response when his theory is supposed to predict magnitude of response in individuals? A critical review may still be fair.

Malone

Malone, too, sees my disagreement with Staddon as related to molecular and molar views of behavior. Some of the same remarks that apply to Shimp’s comments apply also to Malone’s. Unlike Shimp, he shows no sign of having read my 2001 and 2002 papers arguing that the difference is paradigmatic. As a result, he suggests that molar and molecular approaches may both be useful, probably meaning what I would call local and extended analyses. In other ways, however, he is correct. I never liked Hull’s approach, and to the extent that Staddon hopes to resurrect it, I disagree with Staddon.

Malone criticizes my tendency to “ extrapolate ” to large societal concerns in my book *Understanding Behaviorism*, likening it to Skinner’s tendency to write about big issues as if he had the answers. I think I am doing something different. I try to portray these accounts as tentative, probably wrong in detail or entirety, but showing nevertheless that scientific accounts of social issues like freedom, justice, and values are possible. I was answering the charge that a science of behavior cannot deal with such important matters. I think it unwise to ignore societal concerns as if applications were impossible. I also tried to avoid the sin Malone imputes to me, of knowing what is right. Instead I offer a view of culture that is evolutionary (chapters 12 and 13) and argue that design needs to be replaced with experimentation (chapter 14). Malone also is too easy on Staddon in this regard because Staddon (2001) doesn’t follow the strictures that Malone imputes to him. His chapter 5 on justice parallels my own chapter 10, and he adds

to his book a final chapter that is about consciousness and nothing to do with animal behavior.

I share Malone's dissatisfaction over my use of seemingly discrete concepts in my extrapolations, and I tried to substitute more extended concepts in preparing the second edition, which will eventually appear in print.

I disagree with Malone's contention that radical behaviorism forbids theory. In fact, Skinner never forbade theory. I read Skinner as eschewing a priori hypothetical theories (like Hull's) while welcoming those that are driven by data. I agree with Staddon (if it is his view) that behavior analysis has matured to the point that it is ready for theory. Moreover, I like the leaky integrators and have played around with them myself. I consider them, however, to be metaphorical and to have use only insofar as they help us to develop mathematical formulations.

Donahoe

I don't have much to say in response to Donahoe's comments because many of them I agree with and the rest, such as his theory of reinforcement, seem to me outside of the present discussion. I don't accept intervening variables if that means unobservable variables that can never be measured. I think that Staddon and I agree that mathematical formulations predicting behavior may contain parameters that remain fixed in some circumstances and change in meaningful ways in other circumstances. An example is sensitivity in the generalized matching relation (Baum, 1974, 1979). Such parameters may be measures, I would say, not of hidden states, but of long-term, extended patterns of behavior.

Donahoe challenges Staddon and me to produce the evidence that Skinner had the facts about punishment wrong. It is near at hand, for example, in his chapter on punishment in *Science and Human Behavior* (1953). On page 183, he writes, "More recently, the suspicion has also arisen that punishment does not in fact do what it is supposed to do." He goes on to cite approvingly Thorndike's finding that in an experiment with people, saying "right" after a correct response increased its frequency, whereas saying "wrong" after an incorrect response failed to

reduce its frequency. On the next page, he brings up his experiment showing that punishment (paw slapping by the lever, though he doesn't mention it) appeared to produce no reduction in the number of presses made during extinction. He concludes, "Even under severe and prolonged punishment, the rate of responding will rise when punishment has been discontinued, and . . . it has been found that after a given time the rate of responding is no lower than if no punishment had taken place." Then in a section on the effects of punishment, he lists three having to do with respondent conditioning, and repeatedly denies that punishment reduces the behavior that is punished. Research by Azrin and Rachlin showed Skinner's pronouncements about the inefficacy of punishment to be false. Still, we find the same view put forward in *Beyond Freedom and Dignity* (1971, see page 62) and *About Behaviorism* (1974, see page 62).

Finally, I affirm Skinner's stature as the father of behavior analysis, to whom behavior analysts all owe a substantial intellectual debt; that is why I thought Staddon's treatment of Skinner to be so unfair. However, to liken him to a god seems over the top. We have advanced since he wrote *Behavior of Organisms* (1938) and *Science and Human Behavior* (1953), in both the science and the philosophy.

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